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acting as solvents. Others, as liq. soda and liq. potassæ dissolve them readily, and at the same time so affect their coloring matter as to give rise to a succession of tints, usually resulting in some shade of green.

Hot water acts readily as a solvent; cold water, in excess, has a like effect after some hours.

Hydrochloric and sulphuric acids dissolve the crystals easily. Nitrate of silver merely blackens them. Alcohol contracts, swells, and at last granulates them.

In good glycerine, the crystals lose color, but remain intact as to form for many days, so that it may be possible to use this agent as a means of preparing them for the microscopic cabinet.

Otho Franke, who has made numerous researches upon blood crystals, first observed the occurrence of crystals *within* the envelope of the human blood corpuscle. I have frequently seen examples of this phenomenon in the large corpuscles of the sturgeon. The crystals are as usual hexagonal plates, capable of being re-dissolved, so as to give back to view the original outline of the corpuscle. In some instances the nucleus is seen through the crystal, or along side of it, but I am not sure that it is caught or included within the crystal itself, pl. 6.

The last fact to which I desire to call attention, is, I think, quite novel, and certainly very interesting, as illustrative of some points in pathology. When a glass slide containing a group of the crystals is kept for some weeks, the crystals slowly dry, crack in many directions, and by degrees alter in color so as to exhibit very beautiful tints, such as yellow, orange, purple, and varied shades of green.

The same phenomenon may also be seen in the nebulous masses of pigment, in dried blood which has not crystallized. It is to be presumed that these changes of color are due to the slow oxidising influence of the atmosphere. They recal very strikingly the alterations of tint undergone by the leaf in Autumn, and are best comprehended by a glance at the illustrations which accompany this paper. Pl. 2, 3, 4, 5.

March, 1858.

Summary of the Transactions of the Philadelphia Biological Society.

Reported by Henry Hartshorne, M. D., Recording Secretary.

Jan. 18*th*, 1858. Dr. Wm. A. Hammond read a paper "*On the Injection of Urea and other substances into the blood;*"* giving an account of several series of experiments instituted in order to determine the correctness of Frerich's explanation of uræmic intoxication, by the conversion of urea into carbonate of ammonia, and resulting, among other conclusions, in the opinion, that this theory fails to be sustained, and that the carbonate of ammonia is not, itself, more poisonous than urea.

In the brief discussion which ensued, the fact, mentioned in the paper, of the non-appearance of ammonia in the breath after the injection into the blood of urea mixed with vesical mucus, was noted, as being contrary to expectation based on other facts. Dr. Hammond explained that, in his view, the conversion of urea into carbonate of ammonia, which occurs in the presence of mucus out of the body, will not take place in living blood.

Dr. S. W. Mitchell remarked that *healthy* mucus has not been found to hasten this decomposition of urea; but that, out of the body, urea will, without any ferment, undergo spontaneous conversion.

Reduction of temperature by depletion.—Dr. S. W. Mitchell mentioned that, having occasion, recently, in a case of insanity, to take from a patient a very large amount of blood—189 fluid ounces—in a short space of time, he observed

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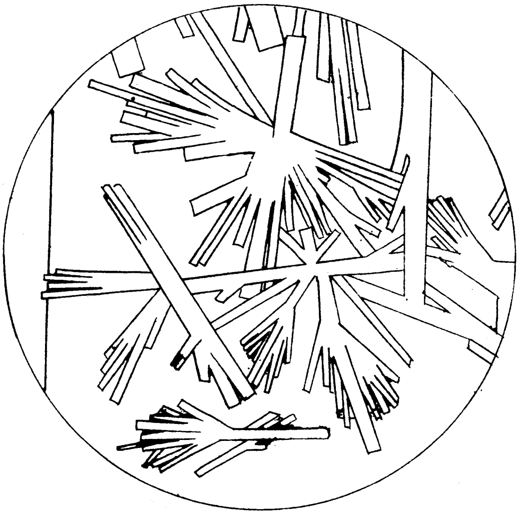
* See N. Amer. Medico-Chiurg. Review, March, 1858.



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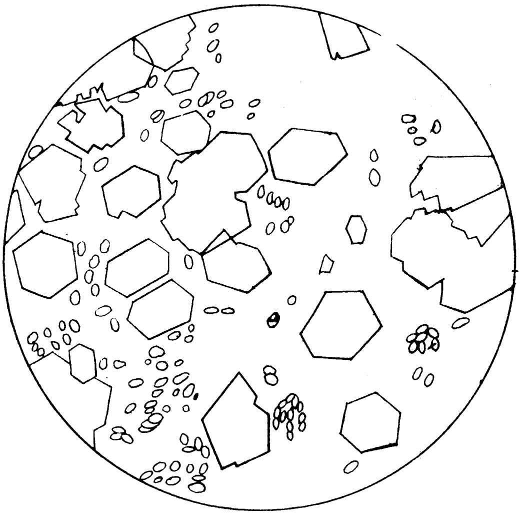

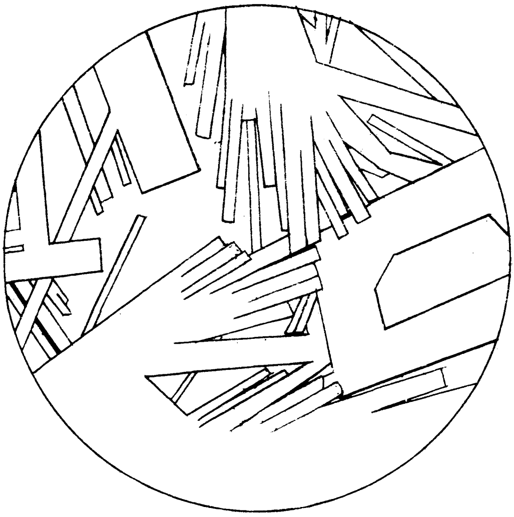

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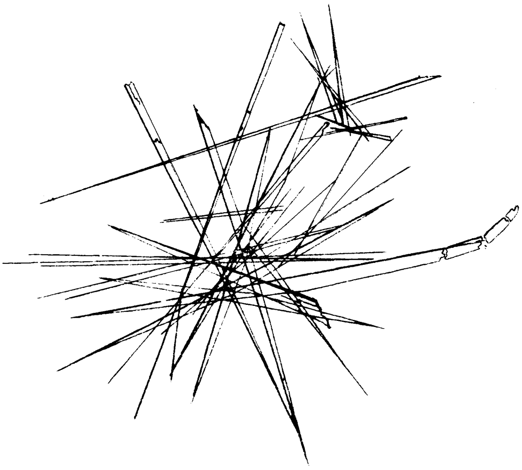

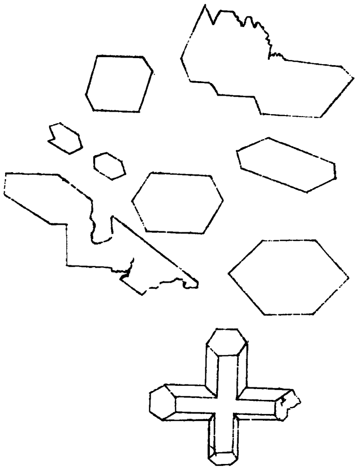

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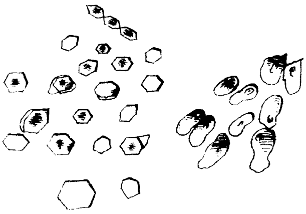

that the temperature of the blood underwent a marked decrease with each depletion. Being, at the first, 100° Fahr., it fell by degrees to 96.2°. During subsequent convalescence it again gradually rose, being at the time of the last examination, 98.7°.

Intestinal absorption.—Dr. JOSEPH LEIDY made the following remarks :

The observation of Goodsir, that the epithelium of the intestine appears frequently to be thrown off in laminæ, exposing the basement membrane so as to absorb directly, has been generally denied by others ; but the true mode of shedding the columnar epithelium of the intestine has not yet been demonstrated. In examining the intestinal canal of insects, as flies, beetles, grasshoppers, &c., the structure of which, although very delicate, is essentially the same as that of the intestine of higher animals, having a columnar epithelium upon a basement membrane, and beneath this a muscular tunic—on placing the mucous membrane upon the object-glass of a microscope while yet living, Dr. LEIDY observed that, when a little water was added, endosmose taking place, the oldest and thinnest cells received the fluid most rapidly, while these were pushed out by the presence of others, thus squeezing the liquid through them. It is, therefore, probable that the epithelium is shed *cell by cell*, the oldest cells being crowded out by the new ones forming beneath. This can be seen by any one under the microscope in the intestine of the common house-fly. The nutritious matter in contact with the epithelial cells passes into them by endosmose, and thence through the basement membrane into the subjacent vessels. Dr. LEIDY has never seen the epithelium stripped off in layers, although he has often watched the process in mice, serpents, salamanders, and frogs, in all of which the mucous membrane has a similar structure. He has, however, very often observed the new cells pushing themselves between the old ones, altering their shape,—the round ones being seen at the bottom, and the elongated oval or pear-shaped ones above.

Feb. 1st. Oxaluria.—Dr. J. CHESTON MORRIS remarked that it is only within the last 35 years that the occasional occurrence of oxalate of lime in the urine has been noticed. At first it was only surmised to exist in this excretion from the fact that calculi were often found to consist partly of it. But soon after the perfection of the microscope, and its general employment in clinical investigation, octohedral crystals of various sizes and great brilliancy were observed, which were proved by chemical investigation to consist wholly of this salt. The discovery of the nature of these crystals would probably have been made earlier, had not observers been led astray by the fact that oxalate of lime, when precipitated ordinarily from solutions of lime-salts by oxalate of ammonia, is amorphous. Lehmann, however, states that when extremely dilute solutions are used, and the precipitate is examined with high powers, minute octohedra may be distinguished. It is obvious, however, that in physiological chemistry some circumstance must exist, modifying the mode of formation of this most insoluble salt, as we find it occurring in quite large and beautiful crystals, as in the cells of various plants, and even in the urine of man and other mammalia. This modifying circumstance I take to be the extremely gradual formation of oxalic acid in the course of the chemical changes constantly taking place in all organic fluids. The formation of oxalic acid is always due to the partial oxidation of the substance when it is procured, whether in the processes of nature, in the vegetable and animal kingdoms, or in our laboratories by the action of nitric acid on sugar. Probably no other substance has undergone so rapid a change of opinion as to its pathological importance. Regarded at first as a sure fore-runner of mulberry calculus, it was supposed to depend upon a peculiar diathesis as its cause. Then it was supposed to be due to the consumption of vegetables containing oxalates ; a view refuted by the utter insolubility of oxalate of lime in any of the fluids of the body, and the consequent impossibility of its penetrating the blood-vessels—yet supported by Wilson and Donné. It has been found to be increased by the use of carbonated liquors and of nitrogenous food ; after epileptic convulsions, in chorea, chronic bronchitis, typhus fever, spermatorrhœa, and dyspepsia. In these diseases, we have either a disturbed respiration or an excessive retrograde tissue-metamorphosis; and to these we must look to discover the cause of the appearance of oxalate of lime in the urine. The changes which the substances introduced into the animal system undergo, are those of progressive oxidation; hence it is easily seen that a diminished supply of oxygen, or an increased waste of tissue, would be accompanied by an imperfect oxidation.

But from being regarded as a pathological constituent of high importance, oxalate of lime has now come to be considered an almost normal substance in the urine. It may be found in almost any specimen of urine which has been allowed to stand until the acid fermentation has occurred. Dr. Golding Bird believes the cause of this to be its deposition from a sort of solution in the urine, an opinion in which I cannot concur, as proof is wanting that it is soluble in *any* urine. It is well known that urine undergoes an acid fermentation, commencing soon after its expulsion from the bladder, and continuing for some days, during which uric and oxalic acids are formed; after a time, however, a second fermentation occurs, in which the urea is converted into carbonate of ammonia. What explanation can we give of the occurrence of these apparently opposite phenomena in the same liquid under the same circumstances? The ferment which acts in both cases is probably the vesical mucus; and the substance changed must be either this mucus, the coloring matter, or the creatin and creatinin, as these are the only other nitrogenized substances present. That it cannot be the mucus is proved by filtering the urine, after which it still ferments, though more slowly. That it is connected with the presence of creatin may be inferred from the fact that Dr. Miltenberger and I (Med. Examiner, 1854) have found crystals of creatin formed by spontaneous evaporation in microscopic specimens of urine containing oxalate of lime. If we take $C_8 H_9 N_3 O_4$ as the composition of creatin, two equivalents of the latter, with 14 of the oxgen would yield 1 equiv. of uric acid, 1 of urea, 2 of oxalic acid, and 8 of water, thus:

$$2\,(C_8 H_9 N_3 O_4) + 14\,O = C_{10} H_4 N_4 O_6 + C_2 H_4 N_2 O_2 + 2C_2 O_3 + 8HO.$$

I suppose some such reaction as this to take place as long as any creatin remains in the urine, thus explaining the acid fermentation; when this has ceased to occur, the urea decomposes into carbonate of ammonia. This will also explain the crystallization of the oxalate; for, as this is a slow process, the oxalic acid is very slowly presented to the lime-salts of the urine, thus fulfilling the condition given by Lehmann as necessary for the formation of the crystals. Prof. Jones, of Georgia, succeeded in obtaining them by endosmose, to which he attributes this formation; but this is obviously only another method of *very gradually* presenting the oxalic acid to the lime, the main condition requisite for their formation.

Dr. HAMMOND observed, that he had frequently found oxalate of lime in the urine, in his own person and that of others, during *health*; large crystals, both of the dumb-bell and octohedral forms being presented. Dr. HAMMOND does not credit the existence of an "oxalate of lime disease," nor of a diathesis, such as is described by some English pathologists, characterized by excess of urea.

Feb. 15th. Dr. T. G. RICHARDSON read an elaborate paper by Dr. GEORGE PATON, of Galt, Canada West, npon the *Functions of the Spinal Cord, as illustrated by experiments on cold-blooded animals;** endeavoring to show occasion for some modification of the theory of reflex action of Marshall Hall, and for the opinion that *perception* is one of the attributes of the spinal cord, and especially of the medulla oblongata.

Dr. LEIDY remarked that the experiments narrated in the paper did not appear to him entirely conclusive, as the movements described might be automatic. This was the view taken by Dr. Leidy of the results observed by him in analogous experiments made several years since, upon frogs, flies, &c. He believed that the conveyance of impressions, in insects, for instance, to the chain of ventral ganglia, should be expected, without supposing perception to produce the apparently voluntary movements. While experimenting at one time upon pigeons, Dr. Leidy kept one alive, after the removal of the cerebrum, for nearly two months. It spent much of the day in walking up and down the room, but never passed through the open door into the room adjoining, the dark shade of which appeared to have the effect, through the retina, of a bar or obstacle. All its motions seemed, to Dr. Leidy, to be automatic and unreasoning; approaching the fire, for example, for warmth, but getting so near as to burn itself, having, repeatedly, to be picked out of the ash-pan. Acts are often set down as evincing consciousness, which are really automatic, such as the darting of a frog at a bright object, or of a fly against a window-pane. The same act will be repeated a thousand times in the same way, without learning by the experiment, it being the mere result of an *impression* on the organs of sense.

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* See N. Amer. Medico-Chiurg. Review, May, 1858.

Mr. W. Parker Foulke suggested an enquiry, in regard to the somewhat complex and determinate movements of newly-born animals, which could not possibly have learned by experience how to perform such acts; if these were automatic, how do they differ from those described in the paper of Dr. Paton? Moreover, in using the term "perceptive act," does Dr. Paton mean that the animal is believed to be *conscious* of the impression through the spinal cord, and to act voluntarily?

Dr. Richardson considered that this was his meaning, and urged, that in some of the experiments related in the paper, sensibility appeared to be shown by the *purposive* character of the movements made. Thus, when an animal, whose brain has been removed, is lightly touched about the neck, and makes a special movement to brush off the irritant, this motion is only one of many movements possible to the same muscles, and one of the *least frequent* or natural to them. We must, therefore, infer sensibility to account for this determinate movement, instead of the more simple and uniform contraction, such as reflex action produces.

Dr. H. Hartshorne believed that different *modes* or *degrees* of irritation of the same surface might produce variable contractions of muscles, under reflex action alone.

Dr. Leidy confirmed this opinion, by examples, in the movements of frogs, flies, &c, from which the brain had been removed.

Dr. Richardson:—Do not these examples really show the possession of sensibility by the cord or ventral ganglia, for which the author of the paper contends?

Dr. H. Hartshorne considered that the statement of the paper, that reflex actions are always uniform under impressions upon the same part, is true only when the impressions are like in degree, and are conveyed by the same nerve-fibres, to the same central ganglia. Thus, the impression of light upon the retina, which, by one reflex circuit causes the contraction of the pupil, will, by another, induce a more general movement of muscles, or the act of sneezing, or the flow of tears. Moreover, a purposive or determinate character in movements does not remove them from the category of automatic actions. Thus, for instance, those called instinctive in lower animals, and others, in the higher, designated as sensori-motor by some physiologists, are still *reflex*, that is, dependent on an *impression* from without, acting through a complex nervous apparatus. Dr. Hartshorne has not, in fact, seen satisfactory evidence of the existence of *will* in any animal except man,—the actions of all the others being explicable upon the idea of more or less complex automatism; and, where the subject is so obscure, it is most philosophical to assume only the one idea or supposition, which will account for the facts, rather than two, as we must do when, in such experiments as those alluded to, we suppose consciousness and will to exist.

Dr. S. W. MITCHELL observed that many very similar experiments to those of Dr. Paton had been performed and recorded by Dr. Dowler, of New Orleans; one of which was more extraordinary than any narrated in Dr. Paton's paper; in which an alligator, whose brain, spinal marrow, and viscera had been removed, yet exhibited movements apparently as intelligently guided as those common to the animal in its natural state.

Dr. HAMMOND had seen a rattlesnake, the head of which had been cut off, and its skin and viscera removed, yet coil itself up and strike.

Dr. HAMMOND then read a paper *On the alterations produced by Intermittent Fever in the excretion of urine, and on the action of the Disulphate of Quinine*;* tending to show, so far as one case, carefully observed, can prove, that the excretion of uric and phosphoric acids is increased during the paroxysm, and that this excess disappears under the use of quinine.

Dr. H. HARTSHORNE made some remarks *On the best means of advancing Biological Science at the present time*, adverting to the danger, which he considered a principal one in the science of the present century, of *logismophobia*, *i. e.* the dread of reasoning, as opposed to simple observation, and expressing the view, that, in every scheme of scientific inquiry, there should be, after the model of Bacon's "New Atalantis," room, not only for observers and experimenters, but also for "depredators," "compilers," and "interpreters of nature." Dr. Hartshorne made an appeal, also, in favor of the organization of a system of *joint investigation*, by observation, experiment and otherwise, by members volunteering for the purpose, in connection with important questions or problems, illustrating this proposition by a series of questions, which might be found capable of determination in such a mode.

Dr. LEIDY, Dr. RICHARDSON and others expressed their approbation of this proposal for organized investigation.

Dr. HAMMOND, in connection with it, gave an account of the German "Verein für gemeinschaftliche Arbeiten," which has been in existence for several years, numbering many hundreds of members, on the Continent and in Great Britain, and issuing, in its archives, a remarkable number of interesting and valuable productions.

March 1*st*. Dr. WALTER F. ATLEE presented a preparation of an acephalous child, and read a paper, giving a description of the monstrosity.†

Dr. LEIDY described a similar one, having neither head nor arms, now in the Museum of the University of Pennsylvania.

Dr. J. C. MORRIS inquired whether the state of the placenta had been examined into in either of those cases? Dr. Morris had noticed, that in several instances of acephalous fœtus, fatty degeneration of the placenta had occurred. Dr. M. had also attended a patient, who was delivered at full term of a fœtus, which had died at the 7th month; fatty degeneration of the placenta was found to be complete. The same mother was afterwards delivered of a child at full term, but which was not more developed than is usual at the 7th month. It has been imagined by some, that fatty degeneration of the placenta at the end of pregnancy, may be the cause of labor; but such a case as that just mentioned, tends to disprove this.

Dr. LEIDY remarked, that the condition of the placenta could not acccount for such a monstrosity as the one exhibited by Dr. Atlee; as this, from its nature, must have been determined in the embryo: having no upper extremities, as well as no head. The upper extremities always pullulate before the formation of the placenta. In this case, also, the placenta must have been healthy enough to furnish nutriment for all parts of the body, which were present.

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* See Amer. Journ. of Med. Sciences, April, 1858.

† See Am. Journ. of Med. Sciences, April, 1858.

Dr. Hammond described a canine monstrosity obtained by him at Fort Riley, Kansas, in which, besides the presence of six legs and several supernumerary toes, there was an entire absence of intestines and anus, the stomach ending in a *cul de sac*.

Dr. Atlee said, that he had, in his paper, said nothing of the *causes* of monstrosities, they being, in fact, altogether *unknown*. Only three modes of causation appear to be possible: 1. Defect in the germ; 2. Emotion, such as fright, &c, in the mother; 3. Accidental violence acting upon the fœtus during gestation. In regard to neither of these can we obtain, at present, accurate knowledge.

Dr. Corse observed, that some modifying causes are occasionally obvious; as, irregular contractions of the uterus, such as are known to occur, and which, by pressure, may act upon the fœtus. Dr. Corse possesses a specimen which illustrates this; in which there is no neck, and only a vesicle in place of the brain. In many cases of acephalous monstrosity, Dr. Corse would suppose external causes to be most probable.

Dr. Leidy thought it difficult to conceive how external causes could act upon the embryo at so early a stage, and while so minute as it must be at the time when such a deformity is produced. Dr. Leidy alluded to the experiments of Newport upon the fecundation of the frog, as suggesting the possibility that the defect might occur, in connection with the act of conception itself.

March 15*th*. Dr. S. W. Mitchell read a paper, entitled "*Observations on the Blood of the Sturgeon,*" illustrated by drawings of crystals obtained from the albuminoid corpuscles of that fish; and containing, amongst other interesting points, new reasons for the belief, that Lehmann's opinion of the coloring matter of the blood being essential to the constitution of blood-crystals, is erroneous.

Dr. Mitchell further remarked, upon his having noticed, with surprise, that, when a drop of sturgeon's blood was left at rest until its *edges* began to dry, the nuclei of its corpuscles, which are easily seen, approached one another; so that, if there were several corpuscles, their nuclei would seem to be in contact. The phenomenon resembled, somewhat, the nummulation of the corpuscles, at the beginning of an inflammation. The mere drying up of the corpuscles did not explain this; the explanation was not obvious.

Dr. H. Hartshorne inquired, whether ordinary *gravitative* attraction might not explain it, as it does the approaching of light particles floating in a liquid to each other, or to the sides of the vessel?

Dr. Mitchell thought not; doubting, even, whether the latter phenomena were explicable by gravitative attraction alone.

Dr. Hammond called attention to the fact, mentioned in Dr. Mitchell's paper, that he had not succeeded in obtaining blood-crystals from the fresh blood of the human finger. Dr. Hammond had made the attempt with blood of the soft-shelled turtle (Trionyx); during the first week he could find no crystals; but, two months afterwards, they were distinctly observable.

Dr. Mitchell had repeatedly failed in the attempt with blood of the water-turtles of the vicinity of Philadelphia. He had, however, in such experiments, observed certain *saline* crystals, which did not undergo granulation and fibrillation before solution; which Dr.M. believes to be a test.

Dr. Morris considered, nevertheless, that the crystals, mentioned by Dr. Hammond, were true blood-crystals; being, as he himself had witnessed, within the blood-corpuscles.

Dr. Leidy remembered having once seen a similar occurrence in the blood of a salamander. He had, also, noticed octohedral crystals within the epithelial cells of a lepidopterous insect, the *Arctia isabella;* which is a caterpillar, found in winter under stones and leaves: each nucleus of an epithelial cell was the centre of an octohedron.

Dr. Mitchell mentioned, that he had found the greatest facility in obtaining human blood-crystals from *pneumonic clots.*

Dr. Morris expressed the opinion that blood-crystallization is a result of change in the albuminoid matter of the corpuscles, under the action of oxygen; the crystals being seldom formed without exposure to the air, or, at least, without time for partial cadaveric decomposition. They are, therefore, *derivative* in their nature.

Leaving this subject,—Dr. Morris stated, that he had been interested, while observing some plants in bloom near a closed window, in finding a deposit of moisture upon the glass, immediately opposite to each *flower;* there being none opposite to the *leaves.* This was not what he should have been led to expect, from the recognized action of the leaves, in concentrating the sap of plants by evaporation. Opposite to the flowers of a Camellia, there had also been found a collection of a viscid sweet substance.

Dr. Leidy supposed this sweet substance to be the secretion ejected by an *aphis*, which sucks the juices of flowers, and throws out continually what is called, as it appears on many plants, *honey-dew;* which constitutes the value of the aphides to ants, whence they have been called *ant-cows.* On grape-vines, in summer, the black aphides abound, and black ants may be seen running up and down, eating this substance, thrown out when the aphis is irritated. It has the power of ejecting it to a considerable distance. On the flowers and other parts of the Ailanthus and Tulip-poplar, drops of it, darkened by dust, are often visible; the Camellia is especially well adapted for their display.

Mr. E. Tilghman exhibited and explained briefly his *application of photography to the construction of goniometers and micrometers.* It consists, essentially, in taking by a microscopic camera a photograph on glass of a graduated semicircle, of proper size to place on the diaphragm of an ordinary Huygheman eye-piece, so as, by super-position, to measure the angle of microscopic crystals. The base of the graduated semicircle may also be divided as a micrometer, so that the same instrument may be used for the two purposes. By advancing or withdrawing the microscopic camera to or from the negative, put up against the light, the graduated semicircle may be adapted to any sized eye-piece. The cost of this construction is very small. Mr. Tilghman anticipates making a fuller report upon the subject at some future time.

Dr. Leidy exhibited a number of *sections of the human cranium*, illustrating the beautiful mechanism by which union of the different parts of the skull is effected, not only in man, but throughout the whole animal kingdom; and which is especially striking before the sutures are co-ossified. One section was horizontal, one vertical antero-posteriorly, and four vertical laterally. In all, minute examination displayed the most exquisite overlapping and interjunction of parts, completing that adaptation to its contents, which is obvious in the form and whole construction of the cranium.

April 19th. Dr. J. Darby read an elaborate paper *on the Supra-Renal Capsules;* advancing the opinion, based on original observation and research, that those organs are so important to the normal condition of the blood, and to the functional actions of the nervous system, as to be almost indispensable to life.

Dr. W. A. Hammond read a description of a *Case of Fatty Degeneration of the Heart, in which death followed the inhalation of chloroform.* The impurity of the chloroform (or chloric ether) used in this case was such, that Dr. Hammond supposed it possible that this might aid in accounting for the fatal result.

Dr. H. Hartshorne, in connection with the same subject, gave an account of some experiments made by himself with the inhalation of *deodorized alcohol.* An inhaling apparatus on the ordinary principle was used, air being introduced only through the alcohol. After breathing through the tube of the apparatus for ten minutes, a feeling of exhilaration was produced, without somnolence or circulatory excitement, but with some diminution of sensibility; and, in one instance, with forcible, though not accelerated palpitation of the heart. The conclusion arrived at was, that any increase in the danger of anæsthesia from chloroform could hardly be expected to result from the addition to it of pure alcohol. This, of course, would not apply to the ethereal or other substances often associated with it.

Dr. HAMMOND mentioned that the preparation alluded to in his paper contained, besides other impurities, a small percentage of *muriatic acid;* very much less, however, than had been reported as contained in a specimen employed in one of the London hospitals.

Dr. J. C. MORRIS supposed that the muriatic acid might have had some influence in causing death : the lungs being congested, and the right side of the heart loaded with blood, while the left side was empty ; as would be the case if the pulmonic circulation were interfered with by an irrespirable substance.

Dr. W. F. ATLEE considered the occurrence of vomiting, shortly before death, to have been an evidence that the brain was powerfully affected, which alone might suffice to cause death.

DR. HAYS observed that it is impossible to account for the deaths which occur under the inhalation of chloroform. Very recently a case has been recorded which strikingly exhibits this: in which the chloroform employed was carefully examined, and found entirely pure; the quantity used was small, the person administering it, one experienced in its use in an English Hospital; and every care was exercised even as to its sufficient dilution with air. The body of the patient was examined after death, and *nothing* was found to explain the result. In cases of undoubted fatty degeneration of the heart, chloroform has repeatedly been given with safety: one writer upon the subject going so far as to say that such degeneration affords *no objection* to its use. In some instances the same patient had inhaled chloroform, from the same bottle, two or three times, without disadvantage, and yet it proved fatal to him at last.

Dr. LEIDY had noticed the same uncertainty or variability of action of chloroform in experiments upon animals. A strong, healthy cat was destroyed by breathing an amount of chloroform, which several others, at the same time, inhaled harmlessly. In his own person, Dr. Leidy had found one of the most marked effects of chloroform to be a total loss of muscular power.

Dr. B. H. COATES believed chloroform to act poisonously, by directly diminishing the action of the heart. The absolute amount of vital power in different individuals or organs cannot be calculated ; but, if the heart be in a state of fatty degeneration, its vitality must be thereby diminished. The rapidity of absorption by the lungs, as demonstrated in experiments recorded by Dr. Coates and others, aids in baffling our calculations. The admixture of other articles with chloroform increases the ambiguity ; and to avoid danger, the greatest care is needed in regard to the *dose.*

Dr. F. G. SMITH inquired whether the state of *consciousness* of the patient was noted in either of the cases alluded to ?

Dr. HAMMOND stated that the man whose case had been narrated in his paper was not conscious during the period immediately preceding death.

Dr. F. G. SMITH observed that the action of chloroform may be traced more directly in the *muscular* than in the nervous organs ; causing a destruction of the irritability of muscular fibre. An analogy is presented to this in some experiments reported by Dr. Rand, in which the leaflets of *mimosa pudica,* exposed to chloroform, lost all their contractibility. Some, at least, of the cases of sudden death under anæsthesia may be explicable upon this principle. Having been repeatedly placed under anæsthetic influence, Dr. Smith had found, that while his consciousness was not at all affected, being able to note the beating of his heart, and to hear and see all that was going on around him, perhaps with even excited sensibility, a total inability to exert the voluntary muscles was produced. The sensation might be described as that of being *flattened out.*

Dr. W. F. ATLEE recalled the suggestive fact, that a few drops of chloroform let fall on a bundle of ciliated epithelium of the frog, will arrest the ciliary motion at once.

Dr. J. C. **Morris** narrated the singular case of a lady, by whom a considerable amount of chloroform was inhaled to relieve morbid vigilance, producing the appearance of sleep, and even stertorous respiration, yet without annulling consciousness; as was shown by her soon re-acquiring the power of motion, and describing the movements made by those about her. There seemed here to be a retention of sensibility, with a loss of voluntary motion.

Dr. **Woodward** had noticed similar phenomena in persons using chloroform by inhalation as an intoxicating agent. He had known several persons, one a medical man, who were in the habit, for months together, of using it in this manner every day after dinner. In all of these there appeared to be a loss of muscular power with retention of consciousness.

Dr. H. **Hartshorne** had formed the opinion that the principal action of chloroform is upon the cerebro-spinal axis, resembling, in its suddenness of effect in fatal cases, some of the experiments of Wilson Philip, and others, in which immediate arrest of the heart's action was produced by destroying the brain with a hammer, or the spinal cord by inserting a heated rod into the canal. The destruction of the spinal marrow will produce, also, general relaxation of the voluntary muscles. At the same time, the cases mentioned suggest the probability that the influence of chloroform may be exerted upon the different nerve-centres in greater or less degree in different cases. It is not impossible. that fatal anæsthesia of the medulla oblongata, destroying life by arrest of respiration, might occur, without prior annihilation of sensorial consciousness.

May 3d. Dr. **Leidy** exhibited a drawing of the *Echinococcus hominis*, commonly known under the name of hydatid. The specimen from which it was taken was found in a tumor seated in the muscles of the right iliac region, which had been supposed to consist of impacted fæces in the colon. The patient had been dead several days, when the body was injected with chloride of zinc; yet two days afterwards the parasites were still alive. None of the injection, however, had come into contact with it, as it had no direct communication with the body. The Echinococcus is the larval form of a tape worm. Dr. Leidy described its mode of propagation and of locating itself in the body.

May 17th. Dr. J. J. **Woodward** read a paper* describing three cancerous tumors, which displayed interesting peculiarities, illustrating one mode of development of carcinoma from a liquid blastema, enveloped in a cyst. The paper opposed the opinion expressed by Rokitansky, that cancer is essentially an albuminosis.

Dr. **Leidy** exhibited a minnow, caught in the Schuylkill, having disease of the scales of the upper part of the head and about the orbit. The scales were dilated, and filled with delicate organic cells, much resembling carcinomatous cells. They were certainly not confervoid or fungous, but were purely pathological, and thus of interest as a specimen of diseased formation in a fish. Disease, in the inferior animals, and even in plants, is deserving of study by medical men, since it may throw light upon the nature of disease in man.

June 7th.—Dr. **Henry Hartshorne** read a paper "*On the bearing of Physiology on Paleontology.*" Referred to a special Committee.

Dr. J. J. **Woodward** made the following verbal communication, in regard to the "*Examination of a fungous growth upon the head of a Hydrargyra fasciata,*" caught in the Schuylkill river, and referred to him by Dr. Leidy at the last meeting of the Department.

This fish presented a dark greenish blue fungous mass upon its head, and several greenish discolorations on various parts of its body. Examination showed the mass to consist of a cryptogamous plant, in the meshes of which loosened and partially disentegrated cells from the epithelium of the scales were abundantly entangled. The dark color was due to the presence of isolated pigment cells identical with those of the pigmentary layer of the scale. The same plant occurred, though less abundantly, on all the discolorations above referred to. The plant resembled more closely the *Torula cerevisiæ* than any other. Like it, it was composed of cells with distinct nuclei, isolated, or disposed in rows, the terminal cells of any row being always of diminished size. But it differed from the Torula in presenting a distinct greenish hue.

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* See Am. Journ. of Med. Sciences, July, 1858.

He has been unable to find it figured in any of the books.

Dr. J. H. Slack alluded to the fungous growth sometimes met with on the sides of fishes, and not unfrequently fatal to them.

Dr. Leidy remarked that this had been ascertained to be the *Achlya prolifera;* a mycelium, with processes terminating in capsules, which emit ciliated and contractile sporules. Weakness of the fish appears to predispose to the attack. It is often found on the gold-fish.

Mr. Queen exhibited a series of beautiful micro-photographs, prepared by Mr. Rehn, presenting views of blood, spicula of sponge, Acarus scabiei, &c.

Dr. J. C. Morris related the particulars of a case of extra-uterine pregnancy, which occurred in the practice of a friend of his, and which will be given to the profession in a short time through a medical journal. The fœtus remained four years in a sac within the abdomen; at the close of this time a fistulous connection was established with the rectum, and the child (of eight months) was brought through the anus by the aid of narrow-bladed forceps.

Dr. Morris also called the attention of the Department to the fact stated by Dutrochet, that the endosmotic current takes place with great rapidity from an acidulated fluid towards a neutral or slightly alkaline one. The amount of acidity exercises a marked influence upon the current; and if it be considerable, destroys it altogether. We are more in a condition now to see the application of this fact in the economy of digestion, than when it was believed that all of the food, after being chymified in the stomach, was sent to the intestines to be converted into chyle, prior to its absorption into the circulatory system. It may be readily conceived that the gastric mucous membrane secretes, especially during digestion, a watery fluid containing a principle capable of splitting, by a sort of fermentation, into lactic acid, such a process being rendered still more probable by the considerable amount of atmospheric air which finds its way to the stomach, mixed with the saliva and food. The presence of this acid will fully account for the occasional occurrence of free muriatic acid, as chloride of sodium is readily decomposed by it at an elevated temperature into lactate of soda and muriatic acid; and the neutral phosphates may in the same way be rendered acid. This would then account readily for the occurrence of the free acid in the gastric juice; while the free acid would again promote a rapid current into the blood-vessels of the fluid now charged to a greater or less extent with the nitrogenised elements of the food.

An explanation is thus afforded also of the nausea and vomiting produced by an excess of free acid in the stomach, quantities of fluid being thus evacuated sometimes, which almost exceed credence. In this case the excess of acid *prevents* the endosmose of the fluid into the blood-vessels, and causes it to accumulate, while the fresh exudations undergo the same change; until, finally, the overburdened stomach disgorges its troublesome load.

If we next consider the acid fluid of muscles as opposed to the alkaline capillary fluid we have a similar condition of things manifested; and the concentrated arterial blood becomes loaded with the water, carbonic acid, and other products of muscular disintegration, a counter-current meanwhile carrying albuminoid material to the fibrilla for their renovation. Here we may find the explanation of the increased nutrition of a muscle that is frequently exercised; the very act of destruction of the tissue by its contraction, causing the endosmotic currents to flow with increased rapidity.

There is some reason to believe, that all secretions are neutral or alkaline when they leave the blood, and only become acid by the fermentative processes set up in them, sometimes with extreme rapidity, as in the case of the urine. We know that oxalic acid must be the result of some such change; and yet *renal* calculi of oxalate of lime are not very rare.

June 21st.—Dr. J. J. Woodward read a paper entitled "*Histological Remarks upon a Secondary Cancer of the Pleura;*" in which evidence was given in support of the assertion that cancerous blastema contains fibrin as well as albumen. The most remarkable points set forth in the examination of the case narrated in the paper, were, 1st, the extent of the pleural surface occupied by the new formation; 2nd, the quantity of the latter and of its exudation; 3rd, the failure of development of its elementary forms beyond the stage of nuclei.

A brief discussion took place upon the subject of the paper, between Dr. Woodward and Dr. Morris; the paper was referred to the Committee on Pathology.

Sept. 6th. Dr. J. J. Woodward described briefly some recent observations made by him upon the cell-wall of pus-cells, separated from their contents by endosmose in urine. A more elaborate communication will be made by him upon the subject, hereafter.

Dr. J. C. Morris remarked upon the results of a post-mortem examination of a horse, which had been affected with frequent urination, along with other symptoms, for some time before death. Bright's disease of both kidneys, in the fatty stage, was discovered.

Dr. Woodward stated that he had observed fatty degeneration of the liver, very often, in animals exposed in our markets and elsewhere. Much of the mutton of our markets, moreover, he had ascertained to be the meat of tuberculous sheep.

In connection with the same subject, Dr. Morris alluded to an epidemic among horses, in Philadelphia, two years ago, which he considered to have had the characters of typhoid fever. Careful examination, in fact, may detect the occurrence of most of our ordinary diseases in the domestic animals.

Dr. Corse had inspected the lungs of three or four hundred sheep, from which he had obtained some ten specimens of well marked tuberculosis. Nearly twenty-five per cent., however, of the sheep exhibited more or less tuberculous deposit; the existence of which he was enabled to trace to an evident connection with bad feeding and keeping. Dr. Corse had also repeatedly found well-marked cancer of the liver in oxen.

Dr. Woodward stated that the general symptoms and local history of tuberculosis in animals are quite parallel to those connected with its occurrence in the human subject; although the proportionate amount of general or sympathetic disorder is somewhat less in the former.

Catarrhal and bronchitic affections frequently occur in cattle. Cysts in the kidney, and calculi in the bladder, are common in cattle. Dogs are especially liable to cancer; but it occurs in cats also. Rats appear to be particularly liable to a number of diseases.

Mr. Tilghman inquired whether artificial or abnormal circumstances could be shewn by observation to account for tuberculosis in animals?

Dr. Woodward affirmed the opinion that such will prove to be the case; although full evidence is at the present time wanting in regard to it. The close resemblance of the normal structures in the anatomy of all the vertebrata should lead us to expect similarity in their pathology and etiology also.

Sept. 20th. Dr. W. A. Hammond read a paper "On the Secondary Formation of Blood-crystals;" which was referred to a special committee.

Dr. Corse exhibited a specimen of morbid growth of hair upon the cornea of an ox.

Dr. Leidy mentioned, in explanation of this monstrosity, that the hairs in the specimen were the result of the excessive development of very minute hairs naturally existing at the inner canthus of the eye, in connection with the caruncula lachrymalis, and corresponding with the nictitating membrane.

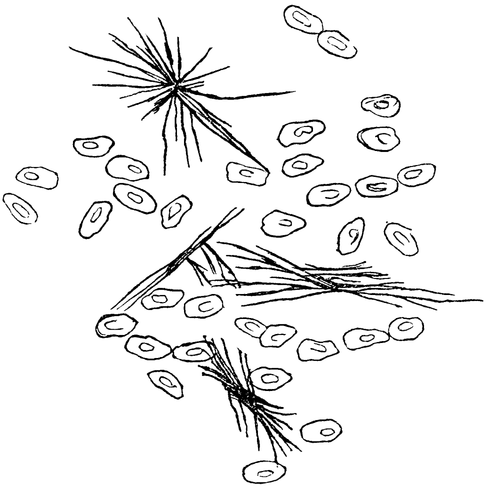
Dr. Hammond stated that he had been requested by the "*Verein für gemeinschaftliche Arbeiten*" to present to the Department the new forms of "*Krankheits Tabelle*" issued by the *Verein*, and to desire some action of the Department thereupon. A Committee was appointed upon the subject, consisting of Drs. Hammond, Woodward and H. Hartshorne.

On the Secondary Formation of Blood Crystals.

BY WILLIAM A. HAMMOND, M. D.

Assistant Surgeon, United States Army.

A few days ago, whilst viewing a slide on which I had four years since mounted some blood-discs of a common prairie snake, (*Eutainia elegans*) I was surprised to find scattered amongst their corpuscles a number of well marked acicular crystals, most of them connected in groups, and radiating from a centre as shown in the drawing.



I had not inspected this slide for about five months, and at that time I am confident the crystals were not present. I had constantly before been in the habit of looking at these corpuscles, but had never previously perceived crystals.

This is the second time it has occurred to me to discover crystals on slides containing blood-discs which must have been formed at a considerable period after the preparation of the specimen. The first instance was on a slide on which were mounted blood-corpuscles of the soft-shell turtle, (*Trionyx ferox*). Crystals were found on this, some months after it was prepared. On a previous occasion, I mentioned this circumstance to the Department.

The discs in both cases were mounted dry, and covered with paper in the